Such, in brief, is the "burden" of this erudite but eminently readable book. It is a fine text-book of open-air history, an attempt to write history "writ large on the face of the country" (p. vii). The available documents are read and expounded *in situ,* so to speak. Archæology, traditions, and folklore "assign their true value to records which have hitherto been loosely read" (p. viii). Accounts of the Scandinavian invasions of western England are read, very properly, in the light of northern antiquities. The first Danish invaders allied themselves with the "bottled-up" "One and Alls," and we learn much about peaceful Danish settlements on the coasts of the Severn. Two archæological maps, many plans and diagrams of camps, and a copious index mark the thoroughness and finish which characterise the whole work. JOHN GRIFFITH.

The British Journal Photographic Almanac, 1914. Edited by G. E. Brown. Pp. 1496. (London: Henry Greenwood and Co.) Price 1*s.* net.

To photographers the approaching end of the year and the beginning of a new one is always heralded by the announcement of the publication of this almost indispensable year-book, which is so familiar to them and a natural fixture in their studios. The copious material contained between the two covers and the useful facts embodied in it has made it a book of reference difficult to part with. The issue for this year follows mainly the lines of its predecessors, but new features of course have been inserted. These, to state them briefly, comprise a glossary of photographic terms, which, no doubt, will be helpful to many a beginner in the subject of photography.

Lists are given of the German, French, and Italian equivalents for the chief appliances and operations, and these should be most serviceable to those who study foreign photographic journals and books, but have no technical dictionary at their elbow. The beginner is also favourably treated with an excellent series of reproductions of negatives incorrectly exposed and developed, which should show him more than words can express what he must avoid; the accompanying text will also prove of service. The epitome of progress, novelties in apparatus, formulæ for the principal photographic processes, miscellaneous information, tables, &c., are as full and complete as ever, and the great number of advertisements are a valuable feature of the volume.

Hazell's Annual for 1914. Edited by T. A. Ingram. Pp. cxiii+592. (London: Hazell, Watson and Viney, Ltd., 1914.) Price 3*s.* 6*d.* net.

IN addition to its revision up to November 25 last, this twenty-ninth issue of "Hazell's Annual" contains a section entitled "Occurrences during Printing." It justifies its claim to give the most recent information on the topics of the day. A section running to some forty pages is headed "The March of Science," and provides a summary of progress made in the world of science during 1913. An index containing 10,000 references makes it easy for the reader to find his way about the volume.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The Pressure of Radiation.

WITH reference to my letter on this subject in NATURE of December 18, the majority of my correspondents complain that, although I may have indicated the possibility of my own view, I have not shown why the simpler relation, that the pressure is always one-third of the energy density, is untenable. As I cannot reply to each individually, I shall be glad if you will allow me space to rectify this omission.

It is generally admitted that the total heat required for the emission (or evolved in the absorption) of unit volume of radiation is the sum of the intrinsic energy density, E/v, and the external work p. By Carnot's principle, this must be equal to $T(dp/dT)$. Whence, if $E/v=3p$, we obtain immediately the fourth power law for full radiation in the usual manner. It would appear, however, by similar reasoning, if E/v is equal to $3p$ for each separate frequency, that radiation of constant frequency should also increase with temperature according to the fourth power law, which is certainly not the case. Either Carnot's principle does not apply, or E/v is not equal to $3p$ for each separate frequency. I have chosen the latter alternative.

It has been shown by Lord Rayleigh, Lorentz, Larmor, Jeans, and others that the electromagnetic equations (from which $E/v=3p$ was first deduced) lead inexorably to Rayleigh's formula,

$$E_\lambda=8\pi R\lambda^{-4}T/N,$$

without the exponential term, for the partition of energy in full radiation per unit range of wavelength λ. This result appears to be true in the limit for long waves and high temperatures, but is otherwise so hopelessly at variance with experiment as to suggest that something may have been overlooked in the application or interpretation of the equations.

Some of my correspondents point out that Nichols and Hull have already shown by experiment that the pressure of a beam of light is equal to the energy density irrespective of wave-length. According to my theory, the mechanical effect which they measured should be equal to the total energy density, $E/v+p$, as deduced from their energy measurements. Their result is in perfect agreement with my theory, but it is not quite such a simple matter (and may even prove to be impossible) to measure p separately from E/v, which is the experiment that I proposed to attack.

H. L. CALLENDAR.

Imperial College of Science, S.W.,
December 27, 1913.

Atomic Models and X-Ray Spectra.

MR. H. G. K. MOSELEY has published in the December issue of the *Philosophical Magazine* a very interesting paper describing his measurements of the wave-lengths of the characteristic X-ray lines of various metals. He has succeeded in calculating the wave-lengths of one-half of the lines he observed, assuming Bohr's atom and supposing the positive charge on the nucleus to correspond to the place of the element in the periodic table as suggested by van den Broek. He concludes that the agreement between calculated and observed wave-lengths strongly supports the views of Rutherford and of Bohr.

It appears to me that Moseley's research really only supports the views of Rutherford and of van den Broek. As I propose to show in detail in a paper to

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be published shortly in the *Verhandlungen der deutschen physikalischen Gesellschaft*, the relation between the wave-length and the positive charge may be obtained in a large number of different ways. For the present it may suffice to point out that it may be derived from a simple consideration of the dimensions of the quantities involved.

The frequency ν can only be supposed to depend upon the magnitude of the positive and negative charges Ne and ne, upon the mass of the moving charge m, upon the distance between the charges r, and, if we wish to introduce the quanta, upon Planck's element of action, h. As Nne^2, m, r, and h must be combined in such a way that the dimension of the resulting quantity is that of one finds

$$(NnML^3T^{-2})^x M^y L^z (ML^2T^{-1})^u = T^{-1}, \text{ or}$$

$x+y+u=0$, $3x+z+2u=0$, and $-2x-u=-1$, whence $y=x-1$, $z=x-2$, and $u=1-2x$.

It is interesting to see what assumptions are necessary to produce an approximate agreement with the experimental data if one inserts various values for x.

If $x=0$ we find $\nu = \text{const.}\ \frac{h}{mr^2}$ the constant being of the order unity as Einstein pointed out. Assuming the characteristic X-rays to be due to the movement of a single electron, we must suppose r to be proportional to $1/N$, where N corresponds to the number of free positive charges on the nucleus found by Rutherford and van den Broek. Roughly speaking, this would be the case if the repulsive force keeping the electrons away from the centre were proportional to $1/r^3$, as suggested by Sir Joseph Thomson. If $x=\frac{1}{2}$ we find $\nu = \text{const}\ \sqrt{\frac{nNe^2}{mr^3}}$. This formula is interesting, as it does not contain h, *i.e.* it may be derived from the ordinary laws of mechanics. It also reduces to Moseley's formula if $r \sim 1/N$.

If $x=1$ the formula is $\nu = \text{const.}\ \frac{nNe^2}{hr}$. If one electron is supposed to oscillate, r must again be assumed proportional to $1/N$ to fit the facts. If all $n=N$ electrons oscillate, r must be supposed to be constant. In this case the formula accounts also for the second series of lines which Moseley's formula fails to do. They may be calculated with great exactitude by putting $\nu = \text{const.}\ \frac{N(N-1)e^2}{hr}$, which corresponds to an atom which has lost an electron.

If we put $x=2$ we find $\nu = \text{const.}\ \frac{n^2N^2e^4m}{h^3}$, which is obviously identical with Moseley's formula, if we suppose only one electron to oscillate. The agreement of Bohr's constant with experimental data is not convincing to my mind in view of the large number of arbitrary assumptions in his derivation.

All the above formulæ are independent of the choice of any special model. They are selected so that the expression for ν is successively independent of e^2, h, m, or r. They would seem to prove that Moseley's figures need not be taken to confirm Bohr's views on the constitution of the atom. The only essential assumption common to all of them is that N should correspond to the place of the element in the periodic table approximately as suggested by Rutherford and van den Broek, and it would seem therefore that this hypothesis only can be said to be supported by Moseley's experiments. F. A. LINDEMANN.

Sidmouth, December 28, 1913.

The Plumage Bill.

SIR HARRY JOHNSTON's plea for the Plumage Bill in NATURE of December 11 will, no doubt, be considered an acceptable contribution by those who believe they possess the mental altitude to which he was born. I venture, however, to suggest that if he and his friends will leave their high mental estate and descend to the plain facts that business men must consider in this lower sphere, he will be obliged to admit that, like the trade, the educated naturalist has much to learn.

He admits the glaring defects of the Plumage Bill, but welcomes the measure as better than none. If he and his friends are able to conceive nothing more than an admittedly bad Bill, that will have no effect on bird-life, he is scarcely justified in his abuse of those who are willing, and trying to solve the problem of saving both the birds and the trade.

His presumption that none but an educated naturalist knows how the skins are procured, or the approximate habitat of the birds, or their right name in English or Latin, does not raise the controversy to any higher plane. Was he not an educated naturalist who bestowed the name of Apoda upon one of the species of paradise bird, believing it be born without feet? In 1908, before the Select Committee of the House of Lords, did not Sir Harry Johnston's friend, Mr. Buckland, declare that the destruction of birds of paradise was at that time so rapid that the species could not last more than two or three years? I see little more in the article which Sir Harry Johnston quotes from *The Times of Ceylon* than a confirmation of the trade statements that the birds of paradise are collected under a system regulating their killing, and that the family is in no danger of extermination. The article shows the valuable commercial asset that Dutch New Guinea possesses, and that its Government is taking full advantage of it under an adequate system of protection.

Mr. Buckland will be surprised to hear that there are so many birds left that this year's production is likely to result in a trade of about 200,000 skins, but he will perhaps be pleased to know that I do not believe it. Both gentlemen should be more concerned in those beautiful specimens said to fetch as much as 40*l*. or more. These are undoubtedly the rare and disappearing species that have no trade interest, but are eagerly sought after for scientific purposes. Even though they be the last survivors of their kind and need some stronger measures than any existing, in order to prevent their utter extermination, supporters of the Plumage Bill have conceived nothing more than a measure that permits their import until none are left, and also prohibits the import of species that are plentiful. L. JOSEPH.

Plumage Committee of the Textile Trade Section
of the London Chamber of Commerce,
Oxford Court, Cannon Street, London, E.C.
December 17.

My reply to Mr. Joseph is as follows:—

I only admit the defects of the proposed Plumage Bill in that it is not sufficiently drastic. But I am always one of those who think half a loaf is better than no bread, and that great restrictive or revolutionary measures of legislation are seldom carried all at once. I should like to see British officers and tourists restrained from destroying the wild mammalian fauna throughout the British Dominions; meantime I welcome sporting licences, close times—any measure which may tend to prolong the existence of interesting wild beasts. So although I should prefer a more complete exclusion from this country of the plumage of rare and remarkable wild birds, I am prepared to accept Mr. Hobhouse's Bill as an instalment of protective legislation.

I continue to assert the utter ignorance of their trade and of the sources and correct nomenclature of their goods which characterise the firms trading the skins and plumes of wild birds. The fact that Linnaeus and

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